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Comment on acp-2021-755

Anonymous Referee #2

Referee comment on "Modeling impacts of ice-nucleating particles from marine aerosols on mixed-phase orographic clouds during 2015 ACAPEX field campaign" by Yun Lin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-755-RC1, 2021

Review of:

"Impacts of ice-nucleating particles from marine aerosols on mixed-phase orographic clouds during 2015 ACAPEX field campaign"

Authors: Lin et al.

Recommend major revisions.

General comment:

Overall, I find the paper and topic interesting and relevant to the microphysics modeling community. The potential impacts of marine sea-salt particles as INP is particularly interesting in, that historically, they have not been considered efficient INP. So, exploring

their impact within a controlled microphysics modeling environment is quite important. As you will see in my specific comments below, I think the authors need to give a fairer assessment of the INP impacts on modeled fields and not attempt to overemphasize effects that appear to be rather minor when viewing the figures. Please show and explain the results in a balanced manner.

Specific comments:

1.Abstract line 36-37: What is the difference between “post-AR” and “after AR”?

2.Introduction line 44: Please be more specific regarding AR impacts on the “western” U.S., specifically when you state that it accounts for 30-50% of the precipitation. By western, do you mean Pacific Coast states?

3.Section 2: Will you please provide the hydrometeor fall speed power law coefficients used in this version of HUCM-SBM for each ice species? I have found the power law coefficients to be quite important in such studies and would like to know what was used in this study.

4.Line 260: The 40% reduction in aerosol number is quite a lot. How much closer to observations do you get with use of the CARB2015 dataset over NEI2011?

5.Section 2.2: How do you get realistic dust transport over the Pacific into California in this scenario with limited spin-up time?

6.Line 297: Why is only larger dust present? Wouldn't the larger dust tend to settle out before the smaller dust particles?

7.Lines 316-323: This discussion on accumulated precipitation hinges on small changes seen in figure 3. It is difficult to see the changes being discussed in this manner. Perhaps figure 3 should include difference plots so that we can more readily see the spillover effects being discussed. As figure 3 is currently presented, all the simulations look very similarwith only minor differences in the details as one would expect when changing a parameterization.

8.Lines 333-334: While long time integration could be impacting cell formation, there are many other model artifacts that could be hindering better prediction compared to the obs. Since you are using this statement to justify moving forward with the analysis, there needs to be better justification or explanation for why the predicted cells differ from the observations. We need to be convinced that the simulations are trustworthy.

9.Lines 353-363: In this section the authors seems to be focused on the improvement to the glaciation ratio in the simulation with sea salt INP while downplaying the large overestimation in water content in figure 5c. Should the simulation with MC18 be considered "better" than the others?

10.Lines 393: The comment on the 330% increase in precipitation is very misleading here. From figure 7a, it can be seen that the 330% increase occurs from a VERY small absolute

increase in precipitation at a time when precipitation rate is very small. These sorts of statements regarding the analysis that over emphasizes a small impact should be clarified or not included in the discussion. The overall changes in precipitation rate due to sea salt INP is quite small as seen in figure 7a, with at most 0.1 mm/hr change.

11.Lines 423-425: While the rain vs snow argument is valid for hydrologic reasons, you have already shown that the accumulated precipitation differences are very small between simulations. So, does this really matter?

12.In general, the discussion of the spillover effect is quite interesting and perhaps should be highlighted rather than over-emphasizing minor changes in precipitation rate. Further, I find figure 9, and the discussion on the glaciation of the cloud, the most fascinating part of this story thus far. It is my opinion that these features should be emphasized earlier in the paper and place it in the context of figure 5c. Does the MC18 simulation produce too much overall condensate while better predicting the relative proportions of water to ice?

13.Lines 528-532: Here you are stating that competition for ice nucleation between dust and marine INP explains the differences in deep cloud occurrence and precipitation, yet you stated earlier that the ice nucleation for dust and marine INP occurs in two different temperature regimes and thus different vertical locations in the cloud. So, how do they compete in this scenario if they are activated in different locations?

14.Lines 538-542: Here you state that cloud dynamics (vertical velocity) is not changed much. Yet in line 524 you discuss invigoration of postfrontal cloud cells. This appear contradictory. Please clarify and discuss how invigoration works in this scenario. You mention the term invigoration several times but have not explained the process.

15.Conclusion: The papers first referenced in the conclusion should be discussed earlier in the manuscript.

Figure 2: Please show panel A on a linear scale rather than log scale. The log scale somewhat minimizes the larger differences in aerosol number between the obs and model. Near 2.8km altitude, I estimate the aerosol concentrations to be 40/cm3 (obs) and 200/cm3 (model).

Figure 10: Why is there no homogeneous ice nucleation in panel D? Is this a contouring issue since the values in panel D are much larger than panel C? Perhaps you could plot panels C and D on a common log scale so that we can see the comparable differences. Also, the figure caption should say "The freezing rates in (c) and (d) …."